

Interactive comment on “Bounce behavior of freshly nucleated biogenic secondary organic aerosol particles” by A. Virtanen et al.

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We would like to thank the reviewer for the comprehensive comments. We have improved the manuscript according to them. Below are the detailed comments for the referee.

Ref. 2 brought out the need to add both measurements below 30 nm size range with solid lab aerosol and also an impactor model. We have performed the comparison measurements with solid ammonium sulphate particles in sub 30 nm size range as well as the estimation of the kinetic energy of the impaction for 20 nm and 40 nm particles and added the analysis in to the manuscript. In the light of the new results, we can more strongly conclude that the decrease of bounce with decreasing particle size in sub 40 nm size range is due to the changes in solidity of the particles. We think

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that this improvement, driven by the comments of the both referees, strengthens the manuscript considerably.

Referee 2 also asked us to add new smog chamber experiments at high RH. This is an important subject which surely needs to be studied. In this study we have used the multistage impactor and, in our opinion, this methodology well-suited for the humidity related research. We have been developing a new single stage method that we are planning to use for studying the impact of relative humidity. We also think that the humidity is not the key factor if we are investigating the possible differences in particle phase and bounce characteristics with decreasing particle size. For these reasons we conclude that the humidity related study is an important subject but not in the scope of this manuscript, which is concentrated on studying the differences of the phase of sub 30 nm particles vs. larger particles.

Ref: “Particle densities derived from AMS and SMPS data (1.0-1.1 g/cm³) are slightly lower than many other smog chamber studies that typically found 1.2-1.5 g/cm³. Could the authors comment on the likely cause for this difference. How was the SMPS and AMS size-calibrated? Given that density measurements for small particles was problematic for the SMP-AMS method, I am somewhat surprised that the SMPS-ELPI method (Ristimäki et al. 2002) was not used for density measurements.” - AMS size calibration was done according to the normalized NH₄NO₃/PSL -based AMS size calibration approach. Four parameters $V_g=600\text{m/s}$, $V_I=10\text{m/s}$, $D_x=1.645\text{m}$ and $b=0.36053$ are applied to correct the particle size in this study. The flow rates of the SMPS were frequently calibrated with a bubble flow meter (Sensidyne’s Gilibrator) in order to minimize errors in the particle size measurements. One possible reason for the lower density in this study than other studies is related to the chemical composition of SOA from oxidation real plant emissions.

The referee suggests that instead of the density gained from AMS-SMPS analysis, we should use the density obtained from ELPI-SMPS comparison. We chose to perform these measurements using the AMS since the ELPI data cannot be relied upon when

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particles are bouncing. We have some data measured with porous (sand blasted) substrates and according to the analysis it seems that the porous substrates we have used considerably decreases the bounce of amorphous SOA particles, but not completely prevents it. This is observed as a widening of the measured ELPI current distributions, which therefore affects the density results gained by the ELPI-SMPS comparison. We are currently investigating substrates with different porosity characteristics to find the material that could completely prevent the bounce and thus would allow us also to perform the density analysis.

The referee brought out the temperature changes in the impactor jet. According to the Arffman et al. (2011) the adiabatic cooling take place immediately after the impactor nozzles. The time scale at which particles spend in the cool region is few microseconds. According to the flow modeling by Arffman et al., the temperature increases again back to the impactor inlet temperatures (in this case it is room temperature) before particles hit the collection substrates. The time scale which particles spend in warm zone above the collection plate is also few microseconds. Thus we don't believe that the cooling plays important role in this context.

Ref: "Is the new point below 30 nm calculated for a single SMPS scan? If so, how much did the size distribution vary over the course of this scan? If the limited resolution of the SMPS may affect the results, then the authors should comment on the uncertainty this impose on the calculated bounce factor" - All the new points in figs 2 and 3 (figs 1 and 2 in the corrected manuscript) are calculated for the single SMPS scan. The maximum growth rate of the particles at 12-30 nm size range was app. 2 nm . Thus the maximum change in SMPS peak value during the one scan (scan time of 120 s) was ~4 nm. The ELPI currents were recorded every second and the bounce factor is calculated from averaged current values (120 s average during each SMPS scan). The effect of this non-ideality on calculated bounce factor values is diminishing.

Ref: "It is well known that bounce from the vaporization oven in the AMS is a critical parameter in quantitative AMS measurements. Bounce is one contributing (often dom-

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inating) factor when a collection efficiency (CE) less than unity is found for the AMS. Was the CE of the AMS quantified? If not I strongly suggest to compare the time dependent CE of the AMS between the different experiments as it should give complementary information to the bounce factor in ELPI." - We have investigated the CE of AMS by comparing the mass concentrations from AMS measurements and calculated from SMPS size distributions assuming density 1.1 g cm⁻³. According to the comparison, the mass from AMS measurements was higher than the mass calculated from SMPS measurements indicating CE values >1. Based on this result we conclude that the CE estimation done by comparing the AMS and SMPS size distributions is not very reliable. In addition, the CE had no clear time dependence behavior. The CE of AMS is very interesting topic but unfortunately we are not able to draw any solid conclusions on that based on our data. - During the CE analysis described above we recognized that an incorrect CE value (0.5) had been applied for sulfate in the data analysis related to the figure 4 a. New results have been calculated using CE=1 for sulfates and organics and therefore Figure 4 has been redrawn.

Ref: "RH of 30% was used in the smog chamber. How does this compare to typical RHs in and downwind boreal forests? I would suggest future chamber experiments be performed with systematically varied RHs or with or without diffusion dryers preceding the ELPI." - According to the paper by Jaatinen et al. (2009), the average RH at Hyytiälä station during new particle formation is approx. 50%. During the events analyzed in Virtanen et al (2010), the RH values were approx. 30%. Thus RH values used in the chamber presents a lower limits of that measured in boreal forests during the formation events, however it is not unrealistically low. We will surely take the referee comment into account when planning the future chamber experiments.

References: Arffman, A., Marjamäki, M., Keskinen, J.: Simulation of low pressure impactor collection efficiency curves., *J. Aerosol Sci.*, 42, 329-340, 2011. Jaatinen, A., Hamed, A., Joutsensaari, J., Mikkonen, S., Birmili, W., Wehner, B., Spindler, G., Wiedensohler, A., Decesari, S., Micrea, M., Facchini, M.C., Junninen, H., Kulmala, M.,

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